(as in chemistry, for the increasing oxidation results, &c.), the coinage of words as fresh needs arose would proceed automatically on rational lines.

This might very well form the object of a special committee of the British Association.

Mr. Oliver Heaviside's system for electromagnetic matters has much to recommend itself for adoption, also, in general physics.

For example, after the plan (1) conduc*tion*, (2) conduc*tance*, (3) conduc*tivity*, we would have, in the case of radiant energy, (1) radia*tion*, (2) radia*tance*, (3) radia*tivity*.

The first is for reference in a general way to the phenomenon in question; the second refers to its amount in appropriate units in any individual case; while the third is suitable for expressing the peculiar action or factor in the phenomenon possessed by different *kinds* of bodies. Thus the radia*tance* from a hot kettle would be the total quantity of energy lost per second. The radia*tivity* would be the quantity of this per square centimetre.

With a view of examining the feasibility of this system, the following list is subjoined. Many of the words appear at first as if they would prove most awkward in practice, but remembering similar fears (which subsequently proved groundless) in electromagnetic matters, one is afraid to say they are due to more than unfamiliarity.

Phenomenon	Amount of	Coefficient of
Absorption	Absorbance	Absorbivity
Attrition	Attritance	Attritivity
Diffusion	Diffusance	Diffusivity
Emission	Emissance	Emissivity
Expansion	Expansance	Expansivity
Extension	Extensance	Extensivity
Friction	Frictance	Frictivity
Gravitation	Gravitance	Gravitivity
Heat	Heatance	Heativity
Inertia	Inertance	Inertivity
Polarization	Polarizance	Polarizivity
Reflection	Reflectance	Reflectivity
Refraction	Refractance	Refractivity
Rotation	Rotatance	Rotativity
Solution	Solutance	Solutivity

Special attention deserves to be called to inertance as a good name for mass, and inertivity for density, to rotatance for moment of momentum, and rotativity for moment of inertia.

GEO. FRAS. FITZGERALD.
FRED. T. TROUTON.

Physical Laboratory, Trinity College, Dublin,
December 5.

On the Nomenclature for Radiant Energy.

IN connection with this subject there are many things to be considered, and one of the most important is the question of radiation and absorption, which requires a completely new nomenclature to get over very serious ambiguities that at present embarrass the subject. It is very necessary to distinguish between what may be called, on Prevost's theory of exchanges, the total radiatance from the actually observed loss of energy by radiation which is, according to this theory, the difference between the total radiatance and the total absorbance. This difference per degree of temperature is very commonly called the radiating power, but this same word is used in quite a different sense when it is attempted to prove, from Prevost's theory of exchanges, that the radiating is equal to the absorbing powers by a consideration of thermal equilibrium. In this latter case the term radiating power means obviously the total radiatance of Prevost's theory.

It may also be worth while calling attention to the theory, given at Nottingham by Lord Rayleigh, as to the absorbivity of rough surfaces being equal to unity. The general idea underlying his investigation is that owing to diffraction the waves amongst the deep corrugations in the surface spread abroad within them, and hardly any of their energy escapes out again. At the time I called his attention to the way a similar theory would explain the radiating power of rough surfaces, as I have taught here for years back. I am mentioning this now to call attention to an experiment of Magnus' mentioned in Jamin ("Cours de Physique," vol. iii. part 3, p. 113, top line, edition 1881; *Pogg. Ann.* vol. cxxiv. p. 476), where I have an old note concerning this theory, and which I had forgotten, to the effect that the radiation from platinised platinum was much greater than that from smooth platinum, but that the increase was chiefly in the ultra-red rays, for that the difference between the two plates was almost completely annulled by a plate of alum. This is what would be expected from the above theory, because corrugations that are small enough to affect the ultra-red radiations might still be too large to be anything but a smooth surface for the visible radiations. There is evidently a good deal still to be done on radiativity.

GEO. FRAS. FITZGERALD.

Physical Laboratory, Trinity College, Dublin,
December 5.

Flame.

I TRUST that, in common with other readers of NATURE, I feel duly chastened by the homily which Dr. Armstrong has addressed to you on the subject of my lecture on "Flame." It is perhaps well that we should be warned from time to time against the sin of dogmatising. The only objection I have to the process is that I should be singled out as a sinner without some good reason being given for the selection. I am charged with forgetting that certain alleged facts "are but phenomena interpreted by our own limited intelligence," and yet I actually wound up my lecture with a quotation from Carlyle, intended to emphasise that very point. If Dr. Armstrong had said that *this* was an "appeal to the gallery," I should not have complained.

I do not feel equal to the metaphysical discussion to which Dr. Armstrong opens the way. I know only of one kind of fact, namely, "phenomena interpreted by our own limited intelligence," and it seems better to spell the thing in four letters than to bury it in phrases that smack of the pulpit.

Now let us see what I have done. I found on burning a hydrocarbon with a limited supply of oxygen, that in the cooled products of combustion all the carbon was oxidised, and that some of the hydrogen was set free. I had been brought up, like Dr. Armstrong, to cherish certain chemical dogmas, one of which was that the hydrogen of a burning hydrocarbon was oxidised before the carbon. I now asked myself what were the grounds for this dogma? It seemed to me to spring from the narrowest view of things, probably from the fact—I mean the by-limited-intelligence-interpreted-phenomenon—that hydrogen gas is easier to set on fire than a lump of charcoal. This was obviously an unscientific conclusion, for the carbon of a burning hydrocarbon is part of a gas, and when it is oxidised it has not, like a lump of charcoal, to be virtually gasified in the act of burning, and so to demand a high temperature and an untold amount of heat. I then read with great profit a paper by Dr. Armstrong, which confirmed my opinion that the heat of combustion of an atom of gaseous carbon, in forming carbon monoxide, must be exceedingly high, and so on all grounds I concluded that there was no *prima facie* reason for assuming that the hydrogen of a hydrocarbon would be oxidised in preference to the carbon. Experiment showed the opposite result; the carbon was oxidised, and I adopted the straightforward explanation, and renounced the old dogma. There were alternative explanations. It was conceivable that the hydrogen burnt first and liberated the carbon, which then acted upon the steam to produce one or both of the oxides of carbon and free hydrogen. We should then have two successive chemical reactions. I pointed out that there was only one piece of indirect evidence in favour of this view, and that has since been contradicted by Prof. Dixon. But Dr. Armstrong appears to suggest the view that the two chemical reactions are simultaneous. Now we know of plenty of chemical reactions which are best understood and remembered if we represent them by two simultaneous equations. When, for instance, zinc is heated with strong sulphuric acid, and we do not get hydrogen, we may explain the apparent anomaly by saying that hydrogen *is* liberated, but that it immediately attacks some of the hot sulphuric acid, producing sulphur dioxide and water. Or we may choose another pair of "normal" reactions which, being supposed to happen simultaneously, will explain the "abnormal" result. But surely no one thinks that the two reactions do proceed simultaneously. I use this method of exposition very largely, but I always tell my students that it is analogous to the treatment of forces in dynamics. We suppose

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a body acted upon by two forces whose direction and intensity may be conveniently represented by the adjacent sides of a parallelogram. The body *really* moves along the diagonal; *virtually* it has a double track, one along each of the adjacent sides of the parallelogram. In like manner, in our hydrocarbon-oxygen system, we may picture two compelling forces, viz. the tendency of oxygen to combine with carbon, and of oxygen to unite with hydrogen. You may, if you please (and this seems a fascinating exercise to Dr. Armstrong), shut your eyes alternately to each force and say, "first the hydrogen gets all the oxygen, and then the carbon snatches some from it," or you may just as well put it the other way about. I simply recorded the fact that the carbon got the most of the oxygen, with an explicit reference to the fact that I was dealing with the cooled products. Before my experiments were published Dr. Armstrong thought that hydrogen got most of the oxygen. He had actually tried to persuade Sir G. G. Stokes and many others to this effect. When my paper appeared he sought to discount the *facts* it contained by flights of polysyllables worthy of a great statesman.[1] Is it not strange that he should now turn to rend the man who relieved him from what he so abhors—a dogma?

The assumption that the products which are collected from a flame may have totally altered in kind during a minute fraction of a second is perfectly gratuitous, and a similar assumption might be made about nearly every reaction in chemistry. Dr. Armstrong might just as well forbid me to say that zinc and hot sulphuric acid give sulphur dioxide, as to say that when a hydrocarbon burns with limited oxygen, the carbon has the preference.

I will not trespass on your space with a discussion of the liberation of carbon in luminous flames. Dr. Armstrong's contentions on that matter are precisely the same character as those I have dealt with above. Dr. Frankland has promised us some new evidence in favour of his theory. I trust I have always treated this theory with respect. I am not bigoted on the subject, though I await Dr. Frankland's promised publication with the same sort of feelings as those with which a Neapolitan might look forward to the reawakening of Vesuvius.

I have now, I hope, given an adequate reply to the question of scientific fact raised by Dr. Armstrong. I will not say much about the imputations he casts upon my scientific honesty. It ought not to be a light thing for a man in Dr. Armstrong's position to accuse a scientific worker of deluding an audience into unsound opinions by means of dazzling experiments, of playing to the gallery; of doing, in short, just those things which are most repugnant to the conscience of an earnest investigator. After years of personal friendship, I know Dr. Armstrong's idiosyncrasies very well, and they are, I imagine, pretty well known to the scientific world in general. I feel compelled, none the less, to ask him either to justify or withdraw his aspersions. I make no appeal *ad misericordiam*, and seek no comforting eulogy. It is a duty Dr. Armstrong owes no less to the scientific world than to myself to state clearly and precisely how I have departed from the standards of diffidence, deliberation, and exactitude that are becoming to a man who is honestly seeking to expound the truth. This, at any rate, is a matter the settlement of which is not contingent upon the arrival of that chemical millennium when we shall recognise "chemical interchange and electrolysis as interchangeable equivalent terms"; and I have the right to ask for an immediate and definite reply to my demand.

ARTHUR SMITHELLS.

December 2.

The Second Law of Thermodynamics.

CLAUSIUS' supposed deduction of the second law from the ordinary equations of dynamics in the form

$$\frac{\partial Q}{T} = 2\partial \log (iT)$$

has been discussed at length by Messrs. Larmor and Bryan in their Report on Thermodynamics for the British Association. They accept the deduction on condition that the system be conservative, that is, that the external as well as the internal forces acting on it are to be derived from a potential.

Now it is admitted that the equation

$$\frac{\partial Q}{T} = 2\partial \log (iT)$$

can be proved for a conservative system with the meaning of i given in the report. But in order that this equation, however true, may express the second law, T and i must be *independent variables*, or (which is the same thing when there is only one controllable coordinate v) T and v must be *independent variables*.

Now the second law implies comparison of two states, in either of which a substance can exist permanently. So if we seek to prove the law, or an analogous law, for a dynamical system, it is essential that we should compare two states of the system in either of which it is in *stationary motion*. One state may have the variables T and v, and the other may have $T + \partial T$ and $v + \partial v$, but there must be *stationary motion* with either pair of values. If then K be the virial of all the forces, external as well as internal, the Clausian equation, $K = 2T$ must hold. But if the system be conservative, as Larmor and Bryan assume, K is a determinate function of v, and the virial equation constitutes a relation between T and v, so that only one of them is independent. For example, a fixed quantity of gas in equilibrium in a vertical cylinder under a piston of mass "m" acted on by gravity. Clearly if T be given, v, the volume, is determinate, or we have only one independent variable. If m be disposable, you may make T and v vary independently, but then the system is not conservative. It seems to me that Larmor and Bryan's equation does not express the second Law.

Prof. J. J. Thomson, in his "Application of Dynamics to Physics and Chemistry," pages 95-100, proves the second law on a certain assumption. And Boltzmann, "Uber die Mechanischen Analogien des zweiten Hauptsatzes," has proved it on, I think, the same assumption. In order to show clearly the nature of the assumption I will begin a proof as follows, treating only the case of T temperature, and v volume. If χ denote the potential of all the conservative forces, p the external force necessary to maintain v constant, we have T being the mean kinetic energy of one of N molecules

$$\partial Q = N\partial T + \partial\bar{\chi} + p\partial v.$$

But by the virial equation

$$p\partial v = \tfrac{2}{3}NT\frac{\partial v}{v} - \overline{\frac{d\chi}{dv}}\partial v,$$

in which $\overline{\frac{d\chi}{dv}}$ is to be distinguished from $\frac{d}{dv}\bar{\chi}$ as explained in Watson's "Kinetic Theory of Gases."

Therefore

$$\partial Q = N\partial T + \tfrac{2}{3}NT\partial \log v + \partial\bar{\chi} - \overline{\frac{d\chi}{dv}}\partial v,$$

or

$$\frac{\partial Q}{T} = N\partial \log T + \tfrac{2}{3}N\partial \log v + \frac{1}{T}\left(\partial\bar{\chi} - \overline{\frac{d\chi}{dv}}dv\right),$$

now make

$$i = \frac{v^{\frac{1}{3}}}{T^{\frac{1}{2}}}$$

(a definite time), then

$$\partial \log T + \tfrac{2}{3}\partial \log v = 2\partial \log (iT),$$

and therefore

$$\frac{\partial Q}{T} = 2N\partial \log (iT) + \frac{1}{T}\left(\partial\bar{\chi} - \overline{\frac{d\chi}{dv}}\partial v\right).$$

Now J. J. Thomson assumes (p. 97) that $\bar{\chi}$, in his notation V, is to be a function of v only, whence it follows that

$$\partial\bar{\chi} - \overline{\frac{d\chi}{dv}}\partial v = 0,$$

as he says, and so

$$\frac{\partial Q}{T} = 2N\partial \log(iT),$$

which I submit as a form of his result (118). I understand Boltzmann in the treatise above cited to make the same

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[1] Here is a quotation from one of his letters to Sir G. G. Stokes: "Regarding the interactions in flames as consisting in a series of simultaneous and consecutive explosions, of which we can only examine the final steady state, it seems to me that the phenomena are necessarily of an excessively complex character, and that their appreciation and successful interpretation must tax our powers of mental analysis in a high degree. It will certainly be unwise at present to infer that the oxidation of the hydrocarbons, or the separation of carbon and also of hydrogen from them, takes place entirely in any one way." This seems to me like saying of a fall downstairs, that it is "a series of simultaneous and consecutive" bumps, &c. so difficult to trace out and presenting so many possible varieties of motion that it is hardly safe to call it a fall *down* stairs at all.

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